

The long-term impact of restorative justice programming for juvenile offenders

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Abstract

While extant research generally supports restorative justice as an alternative to traditional juvenile court processing, much of this research is limited to short-term follow-up periods examining only prevalence of reoffense. In addition, recent meta-analyses point to several study design characteristics, the impacts of which are not well understood. This study compared long-term outcomes of youth referred to restorative justice and traditional juvenile court processing using multiple outcome measures. Specifically, the authors examined the impact of restorative justice referral on prevalence of reoffense, number of later official contacts, and seriousness of later offending behavior over several follow-up periods up to four years post-referral.

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Introduction

Juvenile justice processing over the past twenty-five years has consisted of two distinct trends. On the one hand is the movement for increasingly harsher penalties for serious and violent juvenile offenders, marked primarily by the increasing use of waiver and legislative changes that allow for or mandate adult court processing for younger offenders (Sontheimer, 2001). On the other hand, there has been an expansion of various rehabilitative and/or restorative approaches, including teen or peer courts (Butts & Buck, 2000), juvenile drug courts (Butts & Roman, 2004), restorative justice (Braithwaite, 2002), and other diversionary programs to deal primarily with less serious youthful offenders. While each trend is grounded in unique theoretical orientations, they both provide alternatives to formal juvenile justice system processing, thus lessening case load burdens on juvenile

courts and reducing their potentially criminogenic effects. This study examined the effectiveness of restorative justice as an alternative to traditional juvenile court processing, with an emphasis on long-term recidivism outcomes including prevalence of new offending, the number of new offenses, and the seriousness of later offending behavior. The sample in this evaluation also included a number of juvenile offenders not often included in restorative justice (RJ) programs, specifically those with prior offending histories and those with ‘persons crimes’ (i.e., violent offenses).

Overview of restorative justice

Restorative justice approaches to minor delinquency or criminal violations have gained popularity in the U.S. and elsewhere since the 1970s and are increasingly employed as responses to serious delinquency or adult criminal behaviors (Bazemore & Umbreit, 2001). A range of strategies are generally included under the restorative justice umbrella, such as victim-offender mediation,

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community reparative boards, family group conferencing, and circle sentencing (Bazemore & Umbreit, 2001), although some also include programs utilizing community service or restitution components (e.g., Bonta, Wallace-Capretta, & Rooney, 1998). The overarching purpose of restorative justice programming is restoration of both victims and offenders, as well as the reparation of harm done to the wider community, whose fabric has been negatively impacted by the crime (Smith, 2001). Restorative justice, and various forms of restorative conferencing in particular, involves a series of strategies that attempt to bring together those most affected by a criminal incident (offenders, victims, and community members) in a non-adversarial process to promote offender accountability and repair harms resulting from crime (Bazemore & Umbreit, 2001). While administrative and procedural differences exist, the four basic models of restorative processing (i.e., victim-offender mediation, family group conferencing, circle sentencing, reparative board) share common features including a community-based sanctioning focus, non-adversarial and informal processes, and decision-making by consensus (Bazemore & Umbreit, 2001).

Restorative justice (RJ) advocates often distinguish restorative programs from traditional programs based on the dichotomy between ‘retribution’ (i.e., ‘an eye for an eye’ philosophy) and ‘restoration’ (i.e., repairing the harms associated with crime) (Bazemore, 1998). In addition, the restorative justice response to crime is often contrasted with traditional system processing in terms of differences in the definition of crime, the nature of the proceedings, the primary focus of each approach, and divergent roles afforded to victims (Bazemore, 2000; Bonta et al., 1998; Cormier, 2002; Kurki, 1999; Pranis, 1998; Smith, 2001; Zehr & Mika, 1997). For instance, while traditional justice approaches define crime as an offense against the state, restorative approaches define crime in terms of harm to victims or communities (Cormier, 2002) or a violation of relationships (Zehr & Mika, 1997). Similarly, victims play a limited or passive role in traditional criminal processing, while in restorative approaches they are given a central role and encouraged to actively participate, for instance through in-person meetings with offenders. During these meetings victims are given the opportunity to express their feelings, ask questions of the offender, and articulate the impact of the criminal event. The basic ideas underlying RJ processing focus on attempts to promote offender accountability and change and to meet the needs of victims (e.g., need to be heard and have a say in the outcome of their victimization). This is accomplished by bringing together those most affected

by a crime to discuss the event and its repercussions and to develop a plan to repair harms. Advocates for RJ approaches claim that these features make RJ processing superior to traditional court processing in meeting the needs of victims, strengthening the community, and potentially reducing recidivism by offenders.

Research on the effectiveness of restorative justice

While the potential for reduced recidivism is only one purported benefit of RJ programs, reoffending is a key concern for policymakers considering restorative justice as an alternative to formal court processing. Numerous evaluations of RJ programs have demonstrated high levels of victim and offender satisfaction and compliance with restorative agreements (for excellent reviews see Braithwaite, 2002; Latimer & Kleinknecht, 2000), however, evidence regarding the impact on recidivism is less consistent. Some of the existing research demonstrated reductions in reoffending (Bonta et al., 1998; Hayes & Daly, 2004; Luke & Lind, 2002; Maxwell & Morris, 2001; McGarrell, 2001; Rodriguez, 2005), while other evaluations failed to find significant reductions in recidivism (McCold & Wachtel, 1998; Niemeyer & Shichor, 1996; Roy, 1993; Umbreit, 1994).

An early meta-analysis of restorative justice programming conducted by Bonta et al. (1998) found an average reduction of 8 percent in the reoffending rate of those who participated in programs involving restorative features (e.g., restitution, community service) relative to those who did not. Other recent meta-analyses examined either the effectiveness of justice processes incorporating some restorative features (Bonta, Wallace-Capretta, Rooney, & McAnoy, 2002), a variety of restorative justice programs (Bradshaw & Roseborough, 2005; Latimer, Dowden, & Muise, 2001, 2005), or victim-offender mediation programs for juvenile offenders in particular (Nugent, Williams, & Umbreit, 2003, 2004). Each of these meta-analyses found positive effects for RJ-style programming, with effect sizes including .03 (Bonta et al., 2002), .07 (Latimer et al., 2005), .26 (Bradshaw & Roseborough, 2005), and as high as .30 among studies with stronger methodological characteristics (Nugent et al., 2004).

While this group of meta-analyses generally agreed on the positive effects of RJ processing, studies differed in regards to the impact of various methodological characteristics on the magnitude of effect sizes uncovered. Specifically, there were a number of methodological shortcomings which persisted in the evaluation literature on restorative justice, including varying definitions of reoffense, the length of the follow-up time period studied, and various analytic strategies for

comparing RJ-involved juveniles to those receiving other forms of processing. Each of these factors had been demonstrated to impact the magnitude of effect sizes reported in one or more of the meta-analyses cited above.

Comparison group strategies

Restorative justice programs are generally perceived as diversionary programs, and as such, studies of their effectiveness typically compare RJ participants (who are generally first time offenders without prior offending histories) to participants from other diversion programs (e.g., Roy, 1993) or offenders in the traditional court process (Bonta et al., 1998; Luke & Lind, 2002; McCold & Wachtel, 1998; McGarrell, 2001; Rodriguez, 2005). Some studies had included comparison samples drawn from “true” diversionary programs (e.g., all first-time offenders) (McCold & Wachtel, 1998; McGarrell, 2001), while others had made comparisons of RJ participants to offenders with prior records (e.g., Bonta et al., 1998; Umbreit, 1994). As a result, some of the existing RJ research utilized biased samples (comparing offenders in RJ without offending histories, to those in comparison samples who may have had such histories) which might have produced favorable, but less credible conclusions regarding the impact of RJ. It is important then for evaluations of the impact of RJ programs to utilize appropriate (i.e., matched) comparison samples so that conclusions regarding the effectiveness of RJ interventions can be drawn from differences in recidivism between ‘treatment’ and ‘comparison’ groups that are as similar as possible. When differences do arise, it is also important for evaluations to make use of appropriate multivariate statistical procedures to control for any initial group differences in the groups’ underlying propensities to recidivate.

Another important issue, not adequately dealt with in some existing RJ evaluations, relates to self-selection biases introduced by the voluntary nature of restorative programs. In most cases, once a juvenile offender is deemed ‘appropriate’ for participation in such an intervention, the juvenile is then afforded the opportunity to volunteer for actual participation. As a result, the potential for self-selection bias is introduced if only those most ‘amenable to treatment’ decide to follow through with RJ participation. For this reason, it is crucial for evaluations to examine differences in recidivism rates between those who were assigned to RJ interventions (even if they did not receive them) and those who were assigned to the comparison group. Several studies of RJ effectiveness employed such “intention to treat” designs (e.g., McGarrell, 2001; Sher-

man, Strang, & Woods, 2000), while some analyzed groups based on the type of treatment actually delivered (e.g., Hayes, 2005), and still others based on assignment-treatment status (e.g., McCold & Wachtel, 1998). This distinction is important, as differences in offender motivation may be confounded with treatment effects when groups are analyzed based on whether they chose to actually participate in their assigned intervention. Analyses of “intention to treat” (ITT) or treatment as assigned can provide more credible tests of the impact of restorative justice (Sherman & Strang, 2004) by maintaining the initial group assignment independent of what was actually received. Thus the ITT approach is more suited to disaggregating treatment from motivation effects than are other analytic approaches.

While experimental designs are generally considered the most effective way to control for initial group differences in motivation and offending propensity, only a few RJ evaluations were able to carry out such designs. Even the few RJ evaluations which utilized these powerful research designs were compromised by several factors, including the voluntary nature of restorative processing and possible differential attrition rates. Random assignment to RJ processing often occurs after cases are screened for various case and offender characteristics (e.g., admission of the offense); however, whether the offender then actually receives restorative processing depends on other offender (e.g., remorse, willingness to meet with victim, later offending) and victim (e.g., willingness to meet offender, beliefs regarding restorative processing as adequate resolution) factors. As such, even studies intending to conduct random assignment to restorative versus traditional processing can suffer from self-selection biases. In fact, in one study (McCold & Wachtel, 1998), only 42 percent of cases randomly assigned to restorative conferencing (versus court processing) after initial eligibility screening actually resulted in the juvenile’s participation in a restorative conference.

Yet another limitation existed in the extant literature on RJ programming which relied on quasi-experimental designs in that a number of these studies compared those who completed the restorative justice intervention with those who completed traditional court processing (Rodriguez, 2005), those who were assigned to but did not complete the restorative intervention (Niemeyer & Shichor, 1996), and offenders in other diversionary programs (Roy, 1993). Again, when analyses include as the ‘treated’ group only those juvenile offenders who complete a restorative intervention, the risk arises that desired program effects may be confounded with motivation effects. Specifically, those who successfully complete any program (not just an RJ program) may be

less likely to recidivate, not because of program impacts, but because they were less “serious” offenders or were generally more amenable to treatment than those who failed to complete the intervention. In other words, “successes succeed and failures fail.” This problem may remain when comparing restorative justice completers to traditionally processed completers (e.g., Rodriguez, 2005) if restorative processing itself is seen as more difficult or cumbersome (e.g., emotionally charged) for offenders to complete, thus making the average RJ participant more likely to drop out of his/her program than is the average juvenile assigned to traditional processing (because only the most motivated RJ participants will remain in that sample).

While meta-analyses by Bradshaw and Roseborough (2005) and Latimer et al. (2005) found no differences in effect size by quality of research design (e.g., randomized designs, or the nature of the comparison group), Nugent et al. (2003, 2004) found that variation in effect sizes was largely explained by a twelve-item “group formation methodology” (GFM) scale which included items related to random assignment, matching of clients on key demographic and history variables, and group placement in an unbiased manner, with studies in the top quintile on GFM score finding reductions in reoffense as large as 30 percent (Nugent et al., 2004). In addition, Bradshaw and Roseborough (2005) found stronger effect sizes among studies employing a comparison group of restorative justice failures than among studies comparing restorative justice participants to those in an alternative treatment. The extent to which such variability in study design characteristics (including randomization, method of analyses, and statistical controls for group differences) influence conclusions regarding the effectiveness of RJ programming is an important consideration. As such, additional research designed to overcome some of these lingering shortcomings is needed to further, credibly demonstrate the impact of RJ interventions on recidivism.

Definition of reoffense

Yet another methodological shortcoming in the existing literature on restorative justice programming involves how recidivism is defined in various evaluations. Most RJ studies examined changes in the likelihood (prevalence) of reoffending, while fewer studies addressed new offending rates (Sherman et al., 2000) or the severity of later offending (Nugent & Paddock, 1995). While some studies utilized a broad definition of reoffense, including new officially-recorded contact with the police or the filing of new petitions to the

juvenile court (Hayes & Daly, 2004; Niemeyer & Shichor, 1996; Nugent & Paddock, 1995; Rodriguez, 2005), others employed a more narrow definition of reoffense, including only new convictions or new RJ conference assignments (Luke & Lind, 2002).

These varying definitions are important because, while the meta-analysis by Bradshaw and Roseborough (2005) found no differences in effect size based on how reoffense was defined, Nugent et al. (2003, 2004) again found greater variation in effect sizes in studies employing a broad reoffense definition. In addition, Nugent et al. (2003) found that studies using broad reoffense definitions also tended to have poorer group formation methods and nonequivalent groups, leading the authors to conclude that the impact of victim-offender mediation on these more broadly defined recidivism measures was unclear.

While the narrow definition of reoffense is advocated as a more conservative indicator of program impact leading to fewer ‘false positive’ errors (Nugent et al., 2003), broader reoffense measures may more accurately represent the actual behavior of the offender and may also be less influenced by juvenile justice system factors (e.g., various processing decisions within the juvenile court system). Therefore, it is important to further examine the impact of RJ processing relative to traditional processing on broadly defined reoffense measures in studies with careful comparison selection methods.

Follow-up length

Finally, the extent to which the impact of restorative justice processing changes over time is open to debate, as some research found that program effects disappear or diminish over time (McCold & Wachtel, 1998; McGarrell, 2001), and other research found that effects that maintain over longer periods (e.g., twenty-seven to thirty-nine months) and were not evident using shorter follow-up periods (Luke & Lind, 2002). While the meta-analysis by Bradshaw and Roseborough (2005) found no indication that the length of follow-up period impacted the effect sizes reported among nineteen studies with follow-ups ranging from nine to forty-eight months, the meta-analysis presented by Nugent et al. (2003, 2004) found that effect sizes generally declined over time. For instance, they reported an estimated reduction in offending of 27 percent at six months, but only 9 percent at thirty months (among studies with strong methods employing a narrow definition of reoffense) (Nugent et al., 2003). Further examination of the long-term impacts of restorative

processing are thus important as extant research on the durability of RJ program effects is mixed.

In summary, recent meta-analyses provided support for restorative justice programming as an alternative to traditional justice processing. These same meta-analyses also suggested that the impact of several important study design characteristics, such as comparison/control group formation, analytic strategy, breadth of reoffending definitions, and follow-up time period on the effectiveness of restorative processing are still not completely understood. In addition, while several studies examined likelihood (prevalence) of later offending as a key outcome criterion, less is known about the effect of RJ programming on other reoffense measures, including the number of later offenses and the seriousness of later offending.

Focus of the current study

Given the number of outstanding methodological limitations in the existing literature on the effectiveness of RJ programming, this study examined the impact of restorative justice relative to traditional juvenile justice processing with an eye toward addressing several design issues. In particular, the current study added to the existing literature by examining several recidivism outcomes for youth referred to restorative justice versus traditional juvenile court processing. Specifically, the study examined not only prevalence of reoffending (more broadly defined as any new police contact), but also the number and seriousness of later official contacts, as well as the time until any repeat offending (i.e., new police contacts). These multiple outcomes were also examined over a longer than usual follow-up period (up to four years post-referral).

In addition, this study examined groups based on the intervention they were referred to (ITT; [Sherman & Strang, 2004](#)) in order to more adequately assess the effectiveness of restorative processing while attempting to eliminate the confounding influence of treatment motivation/offending propensity. This approach results in a relatively conservative test of restorative programming as juveniles referred to restorative processing are retained in the treatment group, regardless of whether they actually participated in or completed an RJ intervention. The choice of ITT as a method of analysis may bias results in favor of finding no program effect; however, it more accurately addresses issues of self-selection bias and possible differential attrition issues related to the voluntary nature of restorative programming.

Finally, this study examined a unique program which served not only first-time, nonviolent juvenile offenders,

but also those with prior offenses and current violent offenses. Given the nature of the sample served by the program being examined here, the inclusion of individuals with prior offending history in the comparison sample represented an appropriate, and in fact, necessary design strategy. Given that random assignment to RJ versus traditional processing was not possible in this jurisdiction, multivariate statistical controls for differences in demographic and offending histories of youth assigned to restorative or traditional juvenile justice processing (matched on instant offense) were also employed to further strengthen the credibility of the results generated by this evaluation.

Program description

This study included youth referred to a restorative justice program operating in a mostly rural, midwestern county (population approximately 51,000; the county did include one more urban area, a small city of approximately 30,000). The restorative justice program operates independently of the local juvenile court, although it is funded via state-administered federal monies matched by a contribution from a local county collaborative (made up of various social service agencies). The program began operation in 2000, and its one full-time staff member and several volunteers served an average of fifty youth per year in the first four years of operation. In addition to a four-year criminal justice degree, the full-time staff member completed four years of mediation and facilitation experience prior to program initiation, and received an average of twenty-seven hours of ongoing training per year since 2000. Program volunteers participated in a twenty-four hour training session and an additional fifteen to twenty hour apprenticeship, as well as continuing education activities. While many RJ programs exclude youth with a prior record, youth with an instant offense that includes a violent crime, or cases involving “victimless” crimes, this program attempts to address a broader range of offenders and offense types. In particular, the program is available for offenders with and without prior records, and attempts to address at least some violent crimes (e.g., minor assault), and “victimless” crimes (e.g., traffic violations, status offenses) in addition to property-related offenses (e.g., personal theft, shoplifting, vandalism). As such, the comparison sample utilized in the study also included individuals who had prior offending records in order that it resembled the RJ group as closely as possible.

Youth are referred to the program by several community agencies including victim advocacy groups, schools, law enforcement, the county attorney, and local

courts. After initial referral, restorative justice staff members screen the offender in-person to assess the juvenile's appropriateness for the program. While the juvenile does not have to admit to the offense as a criterion for program referral, such admission is a criterion for continuing in the program beyond this initial screening phase. During the screening process, offenders are informed that participation in the program is voluntary, however, they are also informed that if they choose not to participate their case will be transferred back to the original referring agent (e.g., police), who will then decide how to handle the case. In order to continue in the RJ program individuals must both admit to the offense and be "willing to make it right" by participating in face-to-face dialogue with victims. After the offender's appropriateness is determined, RJ staff attempt to contact the victim(s) and invite them to participate in a face-to-face restorative process.

While the program attempts to facilitate face-to-face dialogue as often as possible, it is best described as a 'hybrid model' or 'variety approach' (Bazemore & Umbreit, 2001) in which staff attempt to match cases with the most appropriate of several possible restorative interventions. Cases may proceed in several directions, based on the staff member's impressions of the case after initial contacts with the offender and victim. For example if the offender is deemed inappropriate for RJ programming or if the victim is unwilling to participate and instead insists on formal processing, the case is generally referred back to the original agency. If the victim is unwilling to meet the offender face-to-face, indirect mediation through an RJ facilitator may occur, or the offender may participate in a victim-impact panel or a community panel (where members of the wider community serve as surrogate 'victims'). In cases where the victim is willing to participate in a face-to-face dialogue, the RJ facilitator proceeds with preparation of both the offender and victim. The facilitator also solicits support individuals for the victim and offender who may also participate in the conference.

In general, the program conforms well to what Bazemore and Umbreit (2001) describe as a 'more-restorative' model in that it is a voluntary program for both victims and offenders, focuses on direct communication between parties, provides advance preparation for parties prior to face-to-face dialogue lasting generally forty-five minutes to one hour, and respects victims' choices throughout case processing. Victim choice is supported throughout by providing victims not only choices in whether to participate, but also regarding the extent of their participation (e.g., direct or indirect mediation). In addition, victims are allowed to choose to have supports

present, and the overall process focuses on consensual agreement regarding the outcome of the case.

Research questions

The current article expands on previous research by exploring four interrelated research questions. While several studies examined the impact of restorative processing on the prevalence of later offending, most included a short follow-up period and few addressed the impact of restorative processing on the extent or seriousness of later offending. To address these limitations, this study incorporated multivariate methods to examine the impact of RJ processing on not only likelihood of new offending, but also number of later offenses, the seriousness of later offending, and the time until new offending.

Specifically, this study began with an examination of whether referral to RJ programming impacted the likelihood of subsequent offending (broadly defined as any new officially-recorded contact with the police, rather than a more narrow definition based on new convictions, for instance) relative to traditional juvenile court referral. Second, the study examined whether referral to RJ processing impacted the number of later official contacts. Third, analyses examined whether referral to restorative processing impacted the seriousness of later offending. Each of these questions was examined at several points in time after referral to respective programs, to assess whether and how the impact of RJ referral changed over a relatively lengthy follow-up time (up to four years in some cases). Finally, survival analysis was employed to examine the impact of RJ processing on the likelihood of reoffending over time.

Methods

Sample

The treatment group in this study included all 213 youth referred to RJ programming during calendar years 2000 to 2003. A comparison sample was developed by selecting youth referred to traditional court processing during the same time period (2000 to 2003) for offenses which were largely similar to those committed by the members of the treatment group. Specifically, the researchers were provided a list of all youth referred to traditional court processing during calendar years 2000 and 2003 in the jurisdiction being studied. From this population, a group of 215 youth were selected for inclusion in the comparison sample based on the type of

referral offense (e.g., property, persons, public order) in order to select a group of youth whose ‘referral’ offenses were generally similar to the RJ group (initial group differences are discussed in more detail below). An examination of both groups revealed that forty-nine of the youth referred to the restorative justice program were actually referred to both restorative processing and traditional juvenile justice processing for the same instant offense. Approximately one-half of those forty-nine clients were referred to restorative justice as a condition of their traditional processing sentence (i.e., probation), while the relationship between restorative and traditional processing for the remainder was less clear (e.g., failure in restorative justice and then assignment back to traditional court processing; successful restorative processing but traditionally processed anyway). Since the focus of this study was on restorative processing as an alternative to traditional processing, those forty-nine cases were excluded from the analysis, resulting in a sample of 164 youth referred exclusively to restorative justice processing and 166 youth referred exclusively to traditional case processing.

Measures

Demographic and current offense history information was provided by restorative justice staff for the 164 youth referred to the RJ program. Instant offense information was used to select a group of youth referred to traditional juvenile court processing. Names of youths in both groups were entered into the local juvenile court data base and demographic and offense history information for each juvenile was retrieved. In addition, official contacts occurring after the date of referral (to either RJ or juvenile court processing) were coded, including date, level (i.e., status offense, misdemeanor, or felony), and type (e.g. status, property, or persons) of offense.

Overall sample characteristics

Table 1 provides demographic, offense history, and follow-up information on the total sample of 330 juvenile offenders included in the study, as well as for each group (RJ and comparison) separately. On average, youth in the sample were 14.7 years of age ($SD=2.34$) upon referral. Seventy-two percent of the sample was White, and the most common minority groups were Hispanic/Latino (17.3 percent) and Native American (6.4 percent). Nearly 74 percent of the sample was male, and most (70.6 percent) came from the small urban area within the county. One-quarter of the youth in the

Table 1
Sample characteristics

Variable	Total sample N=330	Restorative justice N=164	Traditional court N=166
<i>Age at referral***</i>			
Mean (SD)	14.70 (2.34)	13.86 (2.56)	15.54 (1.74)
<i>Race/ethnicity</i>			
White	72.0%	73.2%	70.9%
Hispanic/Latino	17.3%	14.6%	20.0%
American Indian	6.4%	6.1%	6.7%
Other	4.3%	6.1%	2.4%
<i>Gender</i>			
Male	73.9%	77.4%	70.5%
Female	26.1%	22.6%	29.5%
<i>Hometown*</i>			
Rural	29.4%	23.8%	34.9%
Urban	70.6%	76.2%	65.1%
<i>Any prior official contact***</i>	25.0%	15.9%	34.9%
<i># of prior official contacts**</i>			
Mean (range)	.50 (0 to 13)	.29 (0 to 7)	.71 (0 to 13)
<i>Current offense***</i>			
Other	14.8%	9.8%	19.9%
Property	67.0%	77.4%	56.6%
Persons	18.2%	12.8%	23.5%
<i>Follow-up (years)</i>			
Mean (SD)	3.29 (1.16)	3.22 (1.08)	3.36 (1.23)
<i>Follow-up time period</i>			
Through six months	100.0%	100.0%	100.0%
Through one year	100.0%	100.0%	100.0%
Through two years	83.3%	82.3%	84.3%
Through three years	51.2%	49.4%	53.0%
Through four years	32.1%	30.5%	33.7%

† $p < .10$.

* $p < .05$.

** $p < .01$.

*** $p < .000$.

sample had at least one prior officially-recorded police contact, and the average number of prior official contacts for the sample was 0.5 (range 0 to 13). In terms of current offense (offense for which they were referred to RJ or traditional processing), 67 percent of youth were referred for property-related offenses (e.g., property damage, theft), while 18.2 percent were referred for persons offenses (e.g., assault, terrorizing), and 14.8 percent were referred for ‘other’ offenses (e.g., status offenses like alcohol/tobacco violations, disorderly conduct, traffic violations). Finally, the average

follow-up period was approximately thirty-nine months after referral ($SD=1.16$), with 51 percent of the sample followed for a full three years, and 32 percent followed for a full four years after referral.

Despite attempts to draw a comparison sample which was generally similar in the aggregate (particularly on current offense type) to those assigned to RJ processing, several significant initial group differences were found between the two samples (two-tailed significance reported for these group comparisons). In particular, juveniles referred to the RJ program were significantly younger (approximately twenty months) than those referred to traditional juvenile court processing ($F=48.561$, $df=1$, 328 , $p=.000$). Those referred to restorative justice were also significantly more likely to come from the small city (76.2 percent) than juveniles referred to juvenile court processing (65.1 percent, $\chi^2 [1, N=330]=4.95$, $p=.026$). In terms of prior offending history, youth referred to traditional court processing were significantly more likely to have had prior official police contact (34.9 percent) than youth referred to restorative justice (15.9 percent, $\chi^2 [1, N=330]=15.84$, $p=.000$). On average, youth referred to traditional court processing incurred 0.71 prior official contacts (range 0 to 13) whereas youth referred to restorative justice incurred 0.29 prior official contacts (range 0 to 7), and again this difference was statistically significant ($F=8.79$, $df=1$, 328 , $p=.003$). Finally, while the most serious current offense for youth referred to both RJ and traditional processing was most commonly a property-related offense (77.4 percent and 56.6 percent respectively), the comparison group included larger numbers of youth referred for either 'other' or 'persons' related offenses ($\chi^2 [2, N=330]=16.21$, $p=.000$). No significant differences were observed between the groups in terms of race/ethnicity, gender, or follow-up length. Overall, as was the case in a number of existing studies, the comparison sample utilized here was comprised of juveniles who could be considered more 'serious' offenders than was the sample of RJ program participants. As such, multivariate statistics were employed to attempt to control for these initial group differences when examining the impact of RJ programming on recidivism.

Treatment/intervention

While this study retained treatment and comparison groups based on the intervention to which they were referred, rather than based on the intervention actually received, details of the specific intervention components received by both groups were of interest in order to

interpret any demonstrated impact resulting from referral to the RJ program. While complete information on the various program components received was not available, some key information including type of intervention received, conditions of any agreements reached, and completion of that agreement was readily available for the RJ group. Some information related to the services received by the comparison group was missing as well (e.g., specific conditions of probation); however, basic probation disposition information was available for the group of youth referred to traditional court processing.

Nearly all of the youth referred to traditional court processing received a term of probation as a result of their referral to court. The majority of these youth were placed on supervised probation (79 percent), although unsupervised probation (16 percent) and dispositions other than probation (5 percent) were ordered by the court in some cases. Among youth placed on probation by the juvenile court ($N=158$), 32 percent received dispositions involving probation of 90 days or less, 38 percent received dispositions including probation of 91 to 180 days, and another 14 percent were placed on "indefinite" probation (no expiration date set). Unfortunately, information on the specific conditions of these probation dispositions (e.g., requirements to undergo drug testing or various treatment interventions) and any other court-imposed sanctions were not available for the evaluation.

While all youth referred to the restorative justice program received at least an initial in-person contact with the RJ facilitator, 7 percent of RJ cases concluded after receiving only this initial meeting (i.e., the offender was deemed inappropriate for participation in the program). An additional 18 percent of the RJ-referred cases concluded because the victims were unwilling to participate in the program. Direct victim-offender dialogue/conferences (e.g., mediation) was the most common type of RJ interventions; overall 49 percent of the youth referred to RJ processing participated in such a conference, with 95 percent of these meetings involving victim and/or offender support persons (e.g., family members). In addition, 7 percent of cases referred to RJ processing resulted in a victim or community panel (rather than a face-to-face interaction with the direct victim), and 19 percent resulted in an agreement reached by the parties via discussions with the facilitator (indirect mediation; no actual meeting of victim and offender). In 99 percent of cases when some form of RJ intervention (e.g., victim-offender mediation, community panel, and indirect mediation) was deemed appropriate, that intervention was delivered by the program. Similarly, when some form of intervention was deemed appropriate

the intervention led to an agreement being reached in 100 percent of the cases. Finally, when an agreement was reached, in 93 percent of these cases the agreement was fully completed as intended.

Unfortunately, the details of agreements reached as a result of conferences, panels, and mediation via the facilitator were available for only a subsample of forty-eight agreements (40 percent of the total number of agreements). Most of these agreements specified multiple conditions, including verbal and/or written apologies (71 percent of cases), a written report or presentation (13 percent), service work (33 percent), and financial restitution (48 percent).

The details of the treatment/intervention received for each group were of importance for several reasons. For instance, this information is important for comparing the results of this study to other research on restorative justice programming. It is also important in terms of understanding what was actually delivered so that “treatment” effects can be put into some context regarding what intervention occurred to create those effects. Similarly, this information is important for those interested in replicating such programs. On the other hand, it is important to remind the reader at this point that the study utilized here purposefully incorporated an ITT design (Sherman & Strang, 2004) in order to eliminate the confounding influence of treatment motivation/offending propensity which occurs when cases are analyzed based on whether they actually receive the intended treatment. Individuals were retained in the comparison (traditional juvenile court referral) and particularly the treatment (RJ referral) groups, regardless of whether they actually received those assigned interventions. In other words, the RJ group included all youth referred to restorative justice processing, even if they were deemed inappropriate for RJ processing or if the victim in their case was unwilling to participate. For those youth, the ‘intervention’ was limited to initial discussion(s) with the RJ facilitator, which may itself produce some therapeutic benefit. At a minimum, the results presented in this article represent a conservative estimate of the impact of RJ programming, given that not all individuals examined in the RJ group actually received an intensive ‘dose’ of such programming.

Results

A series of bivariate and multivariate analyses were conducted to address the questions of interest: whether outcomes differ for youth referred to RJ compared to traditional juvenile court processing. Bivariate statistics are presented first, followed by multivariate analyses

Table 2
Sample outcomes

Variable	Total sample	Restorative justice	Traditional court	Test statistic	P-value
<i>Any official contact</i>					
Within six months of referral ^a	20.3%	12.8%	27.7%	$\chi^2 = 11.33$.001
Within one year of referral ^a	26.1%	19.5%	35.5%	$\chi^2 = 7.26$.007
Within two years of referral ^b	34.5%	26.7%	42.1%	$\chi^2 = 7.28$.005
Within three years of referral ^c	37.9%	29.6%	45.5%	$\chi^2 = 4.49$.025
Within four years of referral ^d	38.7%	32.0%	44.6%	$\chi^2 = 1.78$.182
<i># Official contacts</i>					
Mean (SD)					
Within six months of referral ^a	.26 (.59)	.16 (.55)	.36 (.69)	F = 10.11	.002
Within one year of referral ^a	.44 (.94)	.30 (.70)	.58 (1.10)	F = 7.86	.005
Within two years of referral ^b	.75 (1.37)	.55 (1.14)	.95 (1.55)	F = 5.99	.015
Within three years of referral ^c	.95 (1.68)	.67 (1.29)	1.22 (1.93)	F = 4.63	.033
Within four years of referral ^d	1.01 (1.92)	.68 (1.23)	1.30 (2.31)	F = 2.85	.094
<i>Most serious new contact</i>					
<i>Within six months of referral^a</i>				$\chi^2 = 13.65$.003
None	79.7%	87.2%	72.3%		
Other	9.7%	4.3%	15.1%		
Property	8.8%	7.2%	10.2%		
Persons	1.8%	1.2%	2.4%		
<i>Within one year of referral^a</i>				$\chi^2 = 9.47$.024
None	73.9%	80.5%	67.5%		
Other	9.7%	5.5%	13.9%		
Property	12.7%	11.6%	13.9%		
Persons	3.6%	2.4%	4.8%		
<i>Within two years of referral^b</i>				$\chi^2 = 11.37$.010
None	65.5%	73.3%	57.9%		
Other	12.0%	5.9%	17.9%		
Property	15.3%	14.8%	15.7%		
Persons	7.3%	5.9%	8.6%		

(continued on next page)

Table 2 (continued)

Variable	Total sample	Restorative justice	Traditional court	Test statistic	P-value
<i>Within three years of referral^c</i>				$\chi^2=5.58$.134
None	62.1%	70.4%	54.5%		
Other	14.2%	8.6%	19.3%		
Property	13.6%	12.3%	14.8%		
Persons	10.1%	8.6%	11.4%		
<i>Within four years of referral^d</i>				$\chi^2=5.37$.147
None	61.3%	68.0%	55.4%		
Other	14.2%	6.0%	21.4%		
Property	12.3%	14.0%	10.7%		
Persons	12.3%	12.0%	12.5%		
<i>Days to first new official contact</i>					
Mean (SD) ^e	329.68 (342.64)	441.67 (416.98)	254.09 (258.15)	F=10.34	.002

Note: P-values presented are two-tailed.

^a Total sample size=330 (164 restorative justice, 166 traditional court cases with at least one year follow-up time).

^b Total sample size=275 (135 restorative justice, 140 traditional court cases with at least two years follow-up time).

^c Total sample size=169 (81 restorative justice, 88 traditional court cases with at least three years follow-up time).

^d Total sample size=106 (50 restorative justice, 56 traditional court cases with at least four years follow-up time).

^e Total sample size=134 (54 restorative justice, 80 traditional court youth with a new official contact).

assessing the impact of restorative justice referral on the likelihood of reoffense, number of new offenses, and seriousness of later offending, controlling for the relevant differences (as presented in Table 1) observed between these two groups. In all cases, the dependent variable of interest involved officially recorded contacts with the police, and thus excluded technical violations or probation revocations which might have been subject to bias resulting from the increased levels of surveillance experienced by youth referred to traditional court processing. In other words, participants in either sample should experience roughly equivalent likelihoods of coming in contact with the police, despite the fact that some members of the comparison sample were being supervised by juvenile probation officers.

Bivariate analyses

Bivariate analyses examining outcomes for the entire sample (N=330) and subsamples of youth referred to restorative justice (n=164) and traditional juvenile court processing (n=166) are presented in Table 2. These outcomes include prevalence of new police contact (yes/no), number of new police contacts, and the type of most serious new police contact within six months of referral and then repeated annually (for up to four years total)

during the follow-up period. Since the sample included youth referred to RJ or traditional court processing over a four-year time period, the length of post-program follow-up varied. As such, sample sizes were smaller as the follow-up time increased, with a total sample of 330 (164 RJ and 166 traditional court) individuals followed up to six months and one year of post-referral, but only 106 cases (50 RJ and 56 traditional court) followed up to four years post-referral. These differences in sample size were not due to attrition in the traditional sense (where individuals drop out of the treatment group over time, as often occurs in similar evaluations), but rather because only a limited number of individuals participated in the RJ program in any given year. For instance, while a total of 164 juveniles had passed through the RJ program between 2000 and 2003 and thus all had at least one year of follow-up data available, only a portion of these youth (approximately fifty) were referred to the RJ program in the earliest years (2000 and 2001) and thus were referred at such a time as to have accumulated four years worth of follow-up time post-program by the time recidivism data was collected (in 2005).

Prevalence of new official contact

Among those individuals with six months of follow-up data available (n=330), 20.3 percent experienced a new officially-recorded police contact. In addition, 26.1 percent of those for whom one year of follow-up data were available experienced a new police contact, as did 34.5 percent of those followed for two years, 37.9 percent of those with three years of follow-up data, and 38.7 percent of those with four years of post-referral follow-up (see Table 2). Among those with each of these lengths of follow-up time, a smaller proportion of youth referred to restorative justice processing experienced a new police contact than did the traditionally processed juveniles, and these differences maintained significance through the sample of cases with three years of follow-up. Specifically, while 27.7 percent of the 'six-month' sample of youth referred to traditional juvenile court processing had a new official contact, this was true of only 12.8 percent of youth referred to restorative justice processing (χ^2 [1, N=330]=11.33, $p=.001$). These differences were also significant among the one, two, and three year follow-up groups, when 35.5, 42.1, and 45.5 percent of youth referred to traditional juvenile court experienced new official police contacts but only 19.5, 26.7, and 29.6 percent of youth referred to restorative processing had. While a greater proportion of traditionally processed youth (44.6 percent) than restoratively processed (32 percent) experienced a new official contact among those with

four years of data, this difference was not statistically significant ($\chi^2 [1, N=106]=1.78, p=.182$).

Number of later official contacts

Among the sample with six months of available follow-up data post-referral, the average number of official police contacts for the entire sample was .26 ($SD=.59$). As Table 2 indicates, the average number of new police contacts among the one, two, three, and four-year groups were .44, .75, .95, and 1.01 respectively. Among each group, the average number of new official police contacts was significantly smaller for youth referred for RJ processing. Specifically, the average number of contacts among the six months group for youth traditionally processed was .36, while the mean number for youth referred for RJ processing was .16 ($F=10.11, df=1, 328, p=.002$). These differences remained statistically significant among the one-year sample, where the mean number of official contacts was .58 for youth referred to traditional processing and .30 for RJ youth ($F=7.86, df=1, 328, p=.005$). The differences were also significant among the sample with two years (.95 versus .55, $F=5.99, df=1, 273, p=.015$) and three years (1.22 versus .67, $F=4.63, df=1, 167, p=.033$) of data available, and the difference was marginally significant among the sample with four years post-referral follow-up (1.30 versus .68, $F=2.85, df=1, 104, p=.094$).

Seriousness of later official contacts

In order to capture information on the level of the most serious reoffense committed by each youth in the groups with various follow-up periods, a four-level ordinal variable was created. Youth who had no official contacts were coded 0 for 'no reoffense.' Status, traffic, and public order offenses were coded 1 for 'other offenses.' Property offenses (coded 2) included crimes such as vandalism, property damage, and theft. Finally, youth were assigned a value of 3 if they had experienced any persons-related offense (e.g., assault, robbery) during the follow-up period.

Within six months of referral to respective programming (RJ or traditional court), 79.7 percent of the overall sample experienced no new official contacts, while 9.7 percent experienced police contact for an 'other' offense, 8.8 percent for a property offense, and 1.8 percent experienced a new police contact for a persons-related offense (Table 2). The proportion of youth with more serious offenses increased over time. Among the sample of individuals with four years follow-up, the most serious offense for the entire sample remained "none" (61.3 percent), however, at this

time 12.3 percent of youth had a new property offense, 12.3 percent a new persons offense, and 14.2 percent had an 'other' offense as their most serious form of recidivism.

Among each sample based on follow-up time, the proportion of youth experiencing more serious offenses was smaller among youth referred to RJ processing than those referred to traditional juvenile court processing, and these differences were significant among the six months, one year, and two-year follow-up period groups (Table 2). For example, among the 'two-year' sample, 8.6 percent of youth referred to traditional court processing experienced new official contact for a persons-related offense, 15.7 percent for a new property-related offense, 17.9 percent for an "other" offense, and 57.9 percent had no new official contacts. Among youth referred to RJ processing, 5.9 percent experienced contact for a new persons-related offense, 14.8 percent for a property-related offense, 5.9 percent for an "other" offense, and 73.3 percent had no new official contacts. These differences were statistically significant ($\chi^2 [3, N=275]=11.37, p=.01$).

Time to first new official contact

Among those youth (in RJ and the comparison sample) with new official police contacts ($N=134$), the average time to the first official contact after referral was approximately eleven months (Table 2). Youth referred to traditional court processing experienced their first new official contact more quickly after referral (average 8.5 months) than those referred to RJ processing (average 15.0 months), and this difference was statistically significant ($F=10.341, df=1, 132, p=.002$).

Multivariate analyses

While bivariate statistics indicated that youth referred to restorative justice had significantly better outcomes than the comparison sample for each of the three recidivism measures examined, these two groups also exhibited significant initial differences on several variables which can be expected to relate to the individual's propensity for reoffending. As such, the following series of multivariate analyses attempted to determine whether recidivism differences remain when these other factors (i.e., demographic characteristics and offense history; see Table 1) were controlled for.

The following sections detail the results of a series of regression analyses which examined the effect of RJ referral controlling for initial group differences. The type of regression was dictated by the nature of the dependent variable, with logistic regression employed for the dichotomous reoffending outcome (i.e., any new

contact), Poisson regression (e.g., count models) employed for the number of new official contacts, and ordinal logistic regression applied to the ordinal variable representing four levels of seriousness of new contacts (i.e., none, other, property, persons). Each of these analyses included controls for age at referral, race (dichotomized as White=0, non-White=1), gender (male=1), residence in the small city (yes=1), number of prior official contacts, and seriousness of current/instant offense (a dichotomous variable representing property crimes and another for persons offenses, with 'other' as the omitted category). Separate regression models are presented in the following sections which represent the effect of RJ referral on each of the outcomes among those cases for which there was at least six months of follow-up data available ($n=330$). This process was repeated on the samples of youth who had accumulated at least one ($n=330$), two ($n=275$), three ($n=169$), and four years ($n=106$) of follow-up time post-program referral. This series of analyses is presented in order to determine whether the impact of restorative justice referral on recidivism maintains over a longer follow-up time than had been utilized in existing RJ research. Multivariate

results are generally presented at the two-tailed significance level; however, since RJ referral was hypothesized to have a negative effect on recidivism the results for the impact of RJ referral on reoffending outcomes in these multivariate models are reported at the one-tailed significance level.

Prevalence of reoffense (new official police contacts)

Results from the logistic regression analyses predicting prevalence of later offending amongst each of the groups (based on length of the follow-up period) are provided in Table 3. The first column lists the predictor variables, with the results of each regression analysis provided in the remaining columns. As the models indicate, living in the small city (versus rural parts of the county) was significantly related to increased reoffense likelihood among those followed up to six months ($p=.022$), and one year ($p=.017$) post-referral, and had a marginally significant effect among those followed for two years ($p=.069$) and four years ($p=.093$) post-referral, but was not significant among those with three years of follow-up time ($p=.32$). The number of prior

Table 3
Logistic regression predicting prevalence of reoffense (later official contacts)

Variable	Six month follow-up group		One year follow-up group		Two year follow-up group		Three year follow-up group		Four year follow-up group	
	B	Exp (B)	B	Exp (B)	B	Exp (B)	B	Exp (B)	B	Exp (B)
	(SE)		(SE)		(SE)		(SE)		(SE)	
Constant	-4.40** (1.43)	.01	-3.58** (1.27)	.03	-1.22 (1.17)	.30	-.45 (1.52)	.64	.63 (2.16)	1.87
Age	.12 (.08)	1.13	.12† (.07)	1.13	.02 (.07)	1.02	-.05 (.08)	.95	-.09 (.11)	.92
Non-White	.30 (.34)	1.35	.07 (.32)	1.07	-.20 (.32)	.82	.02 (.43)	1.02	.39 (.60)	1.48
Male	.44 (.36)	1.55	.34 (.32)	1.40	.12 (.30)	1.13	-.16 (.39)	.86	-1.03† (.54)	.36
Urban	.83* (.36)	2.30	.78* (.33)	2.18	.56† (.31)	1.75	.41 (.42)	1.51	1.03† (.61)	2.80
# of prior contacts	.45** (.13)	1.57	.45** (.13)	1.56	.39** (.14)	1.48	1.06** (.31)	2.89	1.53** (.46)	4.62
Current property	.28 (.41)	1.33	-.11 (.37)	.89	-.11 (.38)	.90	.48 (.59)	1.62	-.51 (.75)	.60
Current persons	.30 (.50)	1.35	-.13 (.46)	.88	-.13 (.46)	.88	.62 (.69)	1.86	.59 (1.00)	1.81
Restorative justice ^a	-.77* (.33)	.46	-.47† (.30)	.63	-.62* (.29)	.54	-.88* (.40)	.41	-.92† (.57)	.40
	$R^2=.179$, χ^2 (8, $N=330$)=39.90, $p=.000$		$R^2=.155$, χ^2 (8, $N=330$)=36.85, $p=.000$		$R^2=.104$, χ^2 (8, $N=275$)=21.54, $p=.006$		$R^2=.211$, χ^2 (8, $N=169$)=28.40, $p=.000$		$R^2=.355$, χ^2 (8, $N=106$)=32.16, $p=.000$	

† $p < .10$.

* $p < .05$.

** $p < .01$.

^a Restorative justice effects tested with one-tailed significance.

contacts was significantly related to increased likelihood of reoffending among each follow-up group (six months $p=.000$, one year $p=.000$, two years $p=.006$, three years $p=.001$, four years $p=.001$). Race (non-White) and seriousness of current/instant offense (property or persons versus other) did not significantly impact the likelihood of new official contacts among any of these groups based on follow-up length, nor were the other demographic factors (age and gender) particularly good predictors of recidivism according to these models.

Central to the focus of this evaluation, referral to restorative justice (versus traditional juvenile court processing) was associated with significantly lower likelihood of reoffense among those with follow-up to six months post-referral ($p=.01$), as well as among those with two ($p=.018$) and three years ($p=.014$) follow-up time, controlling for other factors (all one-tailed significance levels). Restorative justice referral was also a marginally significant predictor of prevalence of reoffense among those with one year ($p=.056$, one-tailed significance) and four years of follow up ($p=.055$, one-tailed significance) controlling for other factors.

Number of later official contacts

Results of the overdispersed Poisson regression analyses predicting the number of official contacts post-referral are presented in Table 4. Results predicting number of new contacts among individuals with each length of follow-up time (up to four years post-referral) are provided in a similar manner to those presented in Table 3 (including RJ effects presented using one-tailed significance). In this series of models, age was significantly related to a higher number of new official contacts among those followed for six months ($p=.045$) and marginally related to recidivism among those followed up to one year post-referral ($p=.096$). Residing in the small city was significantly related to a higher number of new official contacts among those with one year of post-referral follow-up ($p=.016$) and marginally related among those with six months ($p=.053$) and two years ($p=.07$) of follow-up controlling for other factors. The number of prior police contacts was consistently related to number of later official contacts. Among those with six months of follow-up time, juveniles with a higher number of prior official contacts experienced

Table 4
Count model predicting number of later official contacts (overdispersed Poisson)

Variable	Six month follow-up group		One year follow-up group		Two year follow-up group		Three year follow-up group		Four year follow-up group	
	B	t	B	T	B	t	B	t	B	T
	(SE)		(SE)		(SE)		(SE)		(SE)	
Intercept	-3.71 (1.17)	-3.17**	-2.80 (1.05)	-2.68**	-.90 (.94)	-.95	.28 (1.12)	.24	1.73 (1.34)	1.29
Scale	1.06 (.00)		1.27 (.00)		1.48 (.00)		1.58 (.00)		1.47 (.00)	
Age	.13 (.07)	2.00*	.10 (.06)	1.66†	.01 (.05)	.26	-.02 (.06)	-.35	-.07 (.08)	-.88
Non-White	-.03 (.27)	.14	.20 (.24)	.83	.17 (.24)	.71	.24 (.29)	.81	.48 (.34)	1.42
Male	.23 (.27)	.90	.28 (.25)	1.11	.23 (.24)	1.02	.08 (.29)	.03	-.26 (.32)	-.81
Urban	.55 (.28)	1.93†	.66 (.28)	2.40*	.46 (.25)	1.81†	.31 (.31)	1.00	.27 (.36)	.74
# of prior contacts	.11 (.05)	2.28*	.11 (.05)	2.49*	.12 (.05)	2.73**	.14 (.05)	2.88**	.15 (.05)	2.80**
Current property	-.07 (.30)	-.22	-.07 (.27)	-.26	-.00 (.29)	-.00	-.25 (.35)	-.71	-1.20 (.33)	-3.65**
Current persons	-.30 (.40)	-.76	-.49 (.38)	-1.30	-.18 (.37)	-.50	-.40 (.47)	-.85	-.56 (.51)	-1.08
Restorative justice ^a	-.64 (.27)	-2.34**	-.54 (.25)	-2.20*	-.52 (.25)	-2.14*	-.61 (.30)	-2.04*	-.60 (.37)	-1.60†
	LL=-166.10		LL=-148.84		LL=-110.70		LL=-58.64		LL=-32.19	

† $p<.10$.

* $p<.05$.

** $p<.01$.

^a Restorative justice effects tested with one-tailed significance.

significantly more later official contacts ($p=.02$), and this was true in the one ($p=.01$), two ($p=.006$), three ($p=.004$), and four year ($p=.005$) follow-up groups as well. Having a current property offense was significantly related to fewer later offenses only among those with four years follow-up ($p=.000$). Race (non-White) and gender (male) were not significant predictors of the number of later official contacts among any of these follow-up period groups.

Similar to the results in Table 3, RJ referral was related to a smaller number new official contacts among those in each of the follow-up period groups, and this relationship was significant among the six months ($p=.009$), one year ($p=.01$), two ($p=.01$), and three year ($p=.02$) groups as well (all one-tailed significance). Restorative justice referral was also related to fewer new contacts among those with four years of follow-up, but this relationship was only marginal ($p=.054$) at the one-tailed significance. Again note that these smaller

numbers of new police contacts among RJ participants maintained controlling for several factors likely related to reoffending propensity.

Seriousness of later official contacts

Results of the series of ordinal logistic regression analyses predicting the most serious type of reoffense (none, other, property, and persons) are presented in Table 5. Once again results are presented for groups based on length of follow-up period, from six months to four years post-referral (with RJ effects only reported at one-tailed significance). Since the dependent variable was an ordinal categorical variable, positive coefficients indicated that the variable was related to one of the more serious types of repeat offending, while a negative coefficient would indicate that the variable was related to one of the less serious reoffense types.

Table 5
Ordinal logistic regression predicting seriousness of later official contacts

Variable	Six month follow-up group		One year follow-up group		Two year follow-up group		Three year follow-up group		Four year follow-up group	
	B	T	B	T	B	t	B	t	B	T
	(SE)		(SE)		(SE)		(SE)		(SE)	
Intercept 1	4.06 (1.38)	2.94**	2.93 (1.20)	2.43*	.82 (1.12)	.74	-.50 (1.37)	-.36	-2.23 (1.81)	-1.23
Intercept 2	4.90 (1.39)	3.52**	3.57 (1.21)	2.94**	1.45 (1.12)	1.29	.23 (1.37)	.17	-1.46 (1.81)	-.81
Intercept 3	6.87 (1.45)	4.73**	5.29 (1.27)	4.25**	2.80 (1.14)	2.46*	1.31 (1.38)	.95	-5.0 (1.81)	-.28
Age	.12 (.08)	1.54	.10 (.07)	1.40	.01 (.06)	.14	-.06 (.07)	-.74	-.11 (.10)	-1.09
Non-White	.23 (.33)	.69	-.01 (.30)	-.03	-.18 (.31)	-.58	-.15 (.38)	-.39	-.09 (.50)	-.17
Male	.35 (.34)	1.02	.26 (.30)	.85	.07 (.29)	.00	-.06 (.35)	-.18	-.61 (.44)	-1.38
Urban	.79 (.35)	2.25*	.71 (.31)	2.27*	.49 (.29)	1.68†	.28 (.37)	.75	.42 (.50)	.84
# of prior contacts	.30 (.09)	3.14**	.30 (.09)	3.24**	.24 (.10)	2.56*	.34 (.13)	2.62**	.36 (.15)	2.43*
Current property	.03 (.39)	.08	-.33 (.34)	-.97	-.26 (.36)	-.74	-.26 (.49)	-.53	-1.36 (.60)	-2.26*
Current persons	.19 (.48)	.39	-.22 (.43)	-.51	-.22 (.44)	-.51	-.07 (.59)	-.13	-.77 (.80)	-.97
Restorative justice ^a	-.66 (.32)	-2.04*	-.40 (.29)	-1.41†	-.48 (.28)	-1.71*	-.61 (.36)	-1.69*	-.39 (.48)	-.81
	$R^2=.13$, χ^2 (8, N=330)= 34.40, $p=.00$		$R^2=.11$, χ^2 (8, N=330)= 30.71, $p=.00$		$R^2=.07$, χ^2 (8, N=275)= 16.42, $p=.04$		$R^2=.09$, χ^2 (8, N=169)= 14.66, $p=.07$		$R^2=.18$, χ^2 (8, N=106)= 18.50, $p=.02$	

† $p<.10$.

* $p<.05$.

** $p<.01$.

^a Restorative Justice effects tested with one-tailed significance.

Table 6

Cox regression predicting time to re-arrest (among cases with at least three years follow-up time post-program referral)

Variable	B	SE	T	Exp(B)
Age	-.01	.06	-.21	.99
Non-White	-.05	.32	-.16	.95
Male	-.05	.89	-.19	.95
Urban	.19	.30	.65	1.21
# of prior contacts	.23	.06	4.19**	1.26
Current property	-.05	.40	-.13	.95
Current persons	.13	.48	.27	1.14
Restorative justice ^a	-.53	.30	-1.79*	.59

Note: $-2LL=629.06$, $\chi^2(8, N=169)=45.01$, $p=.00$.† $p<.10$.* $p<.05$.** $p<.01$.^a Restorative justice effects tested with one-tailed significance.

In this series of models, residing in the small city was again a significant positive predictor of the seriousness of reoffending among youth followed up to six months ($p=.02$) and one year ($p=.02$) after referral, and as also a marginally significant predictor among those followed up to two years post-referral ($p=.09$). Youth with a larger number of prior police contacts experienced significantly more serious behavior in each of the follow-up period groups (six months $p=.002$, one year $p=.001$, two years $p=.01$, three years $p=.009$, four years $p=.015$). Finally, current property offending was significantly related to less serious behavior, but only among those with four years of follow-up ($p=.02$). Age, race (non-White), and gender (male) were not significant predictors of seriousness of later behavior in any of the follow-up period groups.

Finally, referral to the RJ program was related to less serious behavior for each of the follow-up period groups. This relationship was significant among youth followed up to six months post-referral ($p=.02$), as well as among those followed up to two ($p=.04$) and three years ($p=.04$) post-referral, however, it was only marginally significant among the one year post-referral group ($p=.07$, all one-tailed). Referral to restorative justice was related to less serious later behavior among the sample of juveniles with four years of follow-up data, however, among this group the relationship was not statistically significant ($p=.20$) even at the one-tailed significance level.

Survival analysis

Analyses presented thus far indicate that residence in the small city (versus more rural areas), number of prior official contacts, and restorative justice referral (versus

traditional juvenile court referral) had consistent relationships with later offending as measured with several types of outcome variables. To further illustrate the relationship between RJ referral and the timing of later police contacts, a series of survival analyses (Cox regression) were conducted for all juveniles in each of the follow-up period groups. Survival analysis allows for the “censored” nature of reoffending data, calculates survival probabilities (likelihood of remaining offense-free) over uneven follow-up periods, and allows for a comparison of group survival functions. The results from this series of Cox regressions are presented below.

Cox regression models calculated on samples of juveniles followed for up to six months post-referral, as well as for one, two, and three years post-referral all revealed that RJ referral was significantly related to a lower likelihood of reoffending, while RJ referral was not a significant predictor of reoffense likelihood among the four year follow-up period group ($p=.17$, one-tailed). In the interest of space, only the detailed results for the three-year group are reported (in Table 6 and Fig. 1). As these results indicate, only the number of prior contacts and restorative justice referral were significantly related to the likelihood of reoffending; youth with more prior offenses were more likely to experience a later official contact, while youth referred to restorative justice programming were less likely to experience a later police contact, up to three years post-referral. Specifically, the odds ratio (.59)

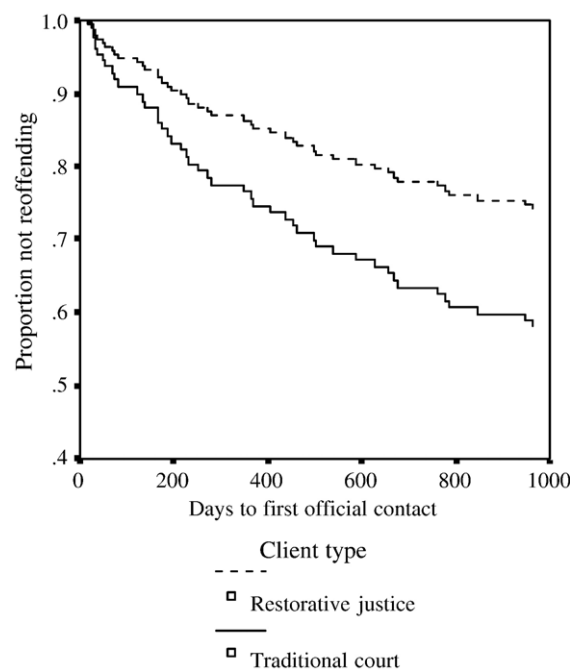


Fig. 1. Survival proportions up to three years post referral.

indicates that reoffense was 59 percent as likely for youth referred to restorative justice compared to those referred to traditional juvenile court processing. The relationship between referral group and reoffending over time is depicted graphically in Fig. 1, with the 'survival curve' for the RJ group represented by the dotted line. As the curves in Fig. 1 demonstrate, juveniles referred to the RJ program reoffended at a slower rate (i.e., survive longer) than did those referred to traditional court processing, again controlling for initial group differences likely related to reoffending propensity.

Discussion

While restorative justice processing had previously received support as an alternative to traditional juvenile court processing in individual evaluations and several meta-analyses, the meta-analyses in particular pointed to several unresolved methodological issues which limited the strength of this conclusion. This study attempted to address several of those limitations. Specifically, it incorporated an examination of groups of juveniles referred to RJ programming who had experienced follow-up periods (up to four years for some cases) which were longer than previously examined. In addition, this evaluation included an analysis of several, broadly-defined recidivism measures and utilized several multivariate approaches to control for individual demographic and offending history characteristics. Finally, this study attempted to improve on the existing literature by utilizing a conservative analytic strategy of comparing groups based on their assigned intervention, rather than what was actually received.

Bivariate results indicated that despite attempts at group-level matching, especially on current offense type, the comparison group appeared to contain some more "serious" offenders than did the RJ referral group (e.g., more prior official contacts and more serious current behavior). Multivariate analyses indicated that juveniles referred to RJ programming fared better than those referred to traditional juvenile court processing on each outcome measure (prevalence, number of later contacts, seriousness of later behavior, time to first reoffense) even when these differences were controlled.

Multivariate regression analyses also revealed a consistent group of relevant variables predictive of success, regardless of the outcome measure used. Residence in the small city within this county was significantly related to poorer outcomes among groups with shorter follow-up periods, and was marginally related in some groups which had been followed for longer periods, a finding that was consistent with existing research linking urbanicity and crime (Braithwaite, 1989; Laub, 1983). The

number of prior official contacts was also a consistent predictor of significantly poorer outcomes, among all follow-up period groups and for each of the three recidivism outcomes examined here. This finding was also consistent with much prior research indicating that prior behavior was amongst the best predictor of future behavior (Andrews & Bonta, 1993).

Finally, with a few exceptions, restorative justice referral was significantly related to better outcomes, and this result appeared relatively consistently in groups followed for as long as three years post-referral. Youth referred to RJ processing were less likely to experience later police contacts, experienced fewer later contacts, and tended to have less serious later behavior than those referred to traditional juvenile court processing, controlling for initial differences observed between the groups. Specifically, RJ referral remained a significant predictor in many of the models controlling for age at referral, race, gender, urban residence, number of prior official contacts, and seriousness of the current offense. The incorporation of several important control variables related to the propensity to reoffend addresses some of the methodological issues identified in earlier meta-analyses, and the finding of significant restorative justice effects despite the more conservative analysis of cases using the 'intention to treat' approach is encouraging.

Related to the decision to analyze cases based on the intention to treat, it is important to note that not all youth in the RJ sample completed restorative justice programming (a factor that may lead to underestimates of the impact of RJ *participation* and/or RJ *completion*). In fact, 25 percent of the youth referred to RJ were excluded from full program participation following initial discussions with the facilitator because these youth were either deemed inappropriate for RJ programming or because victims in these cases were unwilling to participate. On the other hand, all youth referred to the restorative justice program did participate in an in-person discussion with the restorative justice mediator. As such, part of the beneficial impact of the RJ program reported in this article may have occurred simply by these individuals (retained in the treatment group) having participated in this initial discussion (with a focus on the crime and its impact on victims and promoting empathy and offender accountability). On the other hand, the generally supportive results presented here for the effectiveness of RJ referral may also have been 'watered' down by the decision to include cases 'as assigned.' As such it is important to note that the consistently supportive results presented here may well be conservative estimates of the effectiveness of RJ programming, since again, not all of those in the sample of RJ referrals participated in or completed the entire

program. It is also important to note that these positive results were demonstrated using a somewhat more 'serious' sample of juvenile offenders than have been examined in other RJ evaluations (i.e., some with prior records and/or current violent offenses). These preliminary results on the effectiveness of RJ programming for more serious offenders would then suggest that it may be possible to expand the types of potential offenders to whom restorative approaches are applied.

While coefficients for the referral variable all indicated better outcomes for restorative justice-referred clients, some of these regression coefficients failed to reach statistical significance, particularly among those individuals with the longest follow-up period (four years). One explanation for this may be the small number of cases available for analysis at this longest follow-up time period (only 106 cases had been processed in either RJ or traditional court during 2000 and 2001 and thus had this much follow-up time). As such the lack of significant impact among those with four years of available outcome data may be a reflection of statistical power, not a lack of long-term program effectiveness. At the same time, it is possible that this RJ program's impacts do finally wear off after more than three years. Additional research, using even longer follow-up times and larger samples is needed to investigate exactly how long desired RJ effects may maintain.

This analysis incorporated the 'broad' definition of reoffense (i.e., any new police contact) rather than a more 'narrow' definition (e.g., any new conviction), although preliminary analyses of a subset of this data set (not presented) found similar effects regardless of the type of definition used (narrow versus broad). While use of narrow definitions may provide a more conservative indicator of later offending behavior, the impact of alternatives to traditional processing on broader reoffending definitions is also important, as these definitions may be more indicative of actual offending behavior and less influenced by other system factors such as prosecutorial or judicial decision-making. The finding of significant restorative justice effects using the broad definition, along with careful controls for group differences adds importantly to the existing literature. As the meta-analysis by [Nugent et al. \(2003\)](#) indicated, well-documented positive effects were found when narrowly defined outcome measures are used; however, there is a lack of conclusive knowledge on RJ effectiveness on more broadly defined outcomes (largely due to methodological quality of studies employing broader definitions).

While this analysis added to the literature in many ways, several limitations should be noted. Specifically, the current study remained limited in isolating treatment

effects due to the possibility for uncontrolled selection effects. Participants in this program were not experimentally assigned to RJ programming and thus it was not possible to address all potential selection effects (e.g., police perception of some youth as more amenable to restorative processing). While groups were retained as referred and controls for several initial differences in demographic factors and offending history variables were employed, due to lack of appropriate data, the current study was unable to control for some potentially important individual characteristics, such as socioeconomic status, family status, and education level. A direct measure of the individual's motivation for change was also not available for use (as is the case in many correctional program evaluations). At the same time, the analysis of 'intention to treat' was less subject to selection effects than prior analyses focusing on treatment-as-delivered, and the utilization of a number of important controls for differences between treatment and comparison groups helped strengthen the conclusions presented here.

While restorative justice referral appears to be an effective alternative to traditional juvenile justice processing, future research with this data set is planned that will examine whether the impact of restorative justice referral works similarly for different types of youth and whether actual completion of the restorative justice program further contributes to better outcomes. Specifically, plans are in place to examine whether restorative justice referral works similarly for older and younger youth, for males and females, and for youth with various offending histories, and plans are in place to examine whether progression through various restorative justice stages (e.g., conference, agreement, agreement completion) contributes to more positive outcomes, above and beyond referral to restorative processing itself. While much is left to be done in examining the effectiveness of RJ programming, the authors hope that the current results help strengthen the state of knowledge about whether, how long, and on what types of reoffending outcomes restorative justice approaches work.

Acknowledgements

Preliminary results from this study were presented at the Academy of Criminal Justice Sciences annual meeting in Baltimore, February 28–March 4, 2006. The authors wish to thank Jill Wenger, Coordinator, Clay County, Minnesota Restorative Justice Program, and Shelley Ford, Lead Agent, Clay County Family Court Services/Minnesota Department of Corrections for their assistance with this project.

Notes

1. Note that this analytic approach meant that some individuals would be included in more than one sample based on their follow-up time. For instance, those individuals who had up to two years of follow-up also appeared in the sample of individuals who had one year of follow-up data available. This led to samples (based on time at risk, post-referral) which were not completely independent from one another. Several potential problems might arise from this situation, most notably the tendency for “alpha inflation” as repeated tests were performed on samples of data whose members overlapped to some extent. The most typical outcome of alpha inflation is that an apparently significant effect appears simply because the researcher has performed so many tests that a “random” significant effect is bound to appear. Contrary to that scenario (of a single or a few “random” significant results being mistakenly interpreted as meaningful findings), the general conclusions regarding the impact of restorative processing presented in this article appear to be credible, as they are relatively robust over several different modeling approaches (logistic regression, survival analyses, etc.) and over several different follow-up periods.

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